tative analysis. The treatment is marked throughout by a considerable degree of originality, and the book appears agreeably free from the domination of an examination syllabus or of the authority of any particular school. It is unusual to find the determination of silicon in pig-iron or steel in an elementary book, and so also the use of a Hempel gas apparatus; but there is, after all, no good reason why the practical work of elementary students should not be interspersed with exercises of this more technical kind. It is astonishing what sanctity is still attached to the established order of practical chemistry, and it is not the least interesting feature of this book that it is markedly unorthodox. Most teachers will admit that they may profit by carefully inspecting the plans of instruction adopted by their well accredited colleagues, and such a remark may certainly be made of Prof. Hart's little book. A. S.

La Nature et la Vie. By Henry de Varigny. Pp. ii+356. (Paris: Armand Colin, 1905.) Price 3.50 francs.

In a pleasant and easy fashion the writer of this book carries the reader from the beginnings of life to its termination by death. The origin of life on this planet, the vital phenomena of the lower and higher forms of vegetable and animal life, the part played by bacteria in the fertilisation of the soil, the evolution of living forms, parasitism, the multiplication of animals and plants, the beginning of the end, the problem of death, and the immortality of the protozoa are a few of the subjects dealt with. The book may be recommended as a good popular introduction for the educated but non-scientific reader to general biological problems.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

A Plea for Absolute Motion.

Newton believed in the possibility of absolute motion (*i.e.* motion in space not necessarily relative to other material bodies), founding his argument on the fact that the rotation of a planet might be detected by experiment on the planet itself without reference to outside bodies. Newton's reasoning is unanswerable, but it only takes us part of the way. Though it proves that using the principle of gyrostatic action we can determine direction in space absolutely, it fails to distinguish one parallel line from another. We can only observe relative motion. This statement, which no one doubts, is generally taken to be synonymous with the assertion that nothing but relative motion will ever be known. So firmly is this generalisation rooted in the present generation of philosophers that I am afraid the expression of a contrary opinion will only result in placing its author on the "Index" of De Morgan's Budget of Paradoxes.

It is therefore with considerable hesitation that I venture to raise the question whether we are not most of us in our innermost hearts believers in absolute motion, and whether a good deal of the persistence with which we try in our lectures to prove that no meaning can be assigned to absolute motion does not arise out of the desire to repress our own rebellious doubts. As regards the direct evidence of observation we are all agreed, but if from the outset we limit the results of reasoning to that which may directly be controlled by experiment, we must throw overboard a good many theories which are firmly believed in by men of science. I will try to show that it is almost impossible to exclude the idea of absolute motion from our discussions, and that some of our scientific definitions tacitly admit it.

The observed motion of the solar system through the stellar universe has frequently been introduced into the discussion of relative motion, but I do not think that its full importance has been recognised. The thesis I wish to maintain is that the question whether our solar system possesses velocity not only relatively to the stellar universe, but absolutely in space, constitutes a definite problem to which a scientific meaning can be attached. It is immaterial to my purpose whether our present observations are sufficient to allow us to draw any definite conclusions. If the validity of the question itself is admitted, my point is gained.

In order to free the main issue from the uncertainties arising out of the imperfections of our observations, I will base my argument on an ideal condition of the universe which resembles the real universe sufficiently to be admitted as a possibility. The displacement of a star relative to the solar system may be determined in two ways. While telescopic observations give us the angular motion in a plane at right angles to the line of sight, spectroscopic observations allow us to determine radial velocities. To determine velocities by means of the telescope we require to know the distance of the stars, but the determination of parallax is a question of instrumental perfection and of long-continued observation. We commit, therefore, no error in principle if we imagine the parallaxes of the stars in our ideal universe to be known, so that the combination of telescopic and spectroscopic observations can determine the relative velocity in magnitude and direction.

It is a matter of history that telescopic observations alone have led to the conclusion that the solar system moves relatively to the stellar system towards a point which, as fixed by Prof. Newcomb's discussion, has a right ascension of 277°·5 and a declination of 35°. Taking this point as apex, Prof. Campbell divided the heavens into eighteen zones, obtained by drawing circles of latitude at a distance of 10° with the apex as pole. In every one of the zones which had a smaller apical distance than 90°, the average motion was one of approach to the sun, and in every one of the zones having an apical distance greater than 90° the motion was one of recession from the sun. A complete discussion gave for the line of direction, as obtained by the spectroscopic method, R.A. 277°·5, dec. 20°, the right ascension agreeing exactly with the value deduced by Newcomb, though the declination differs materially. The relative velocity found was about 20 kilometres per second.

We may now idealise this observed universe so as to simplify the argument, and bring out its essential points. Divide the heavens into a number of compartments. Let in each compartment the relative velocities be measured for a large number of stars combining the spectroscopic and telescopic method. Let u be the average velocity of each group relatively to the solar system, so that the velocity of each star in the group can be represented by $u+v$, both quantities being vectors. For the sake of argument, assume that u is the same for all groups, and that v within each group is distributed according to the law of errors. As regards v, there is no predominance of any direction (otherwise u would be affected), and its magnitude will be distributed about its mean value in a manner which we will take to be the same for all groups. The question arises: How should we interpret such observations if the facts were as stated?

It is not sufficient to say that the observations would prove a relative motion $-u$ of the sun with respect to the stellar system, for this would only represent a small part of the facts. The important point brought out by the observations is that the relative motion is observed to be the same for the mean point in each one of a great number of groups of stars. The fact that within each group the distribution follows the law of errors leads to the conclusion that the groups are independent systems, and I put the question thus: Does it require an explanation why all these independent systems should have the same vector u imposed upon them? If you admit the validity of this question, if you begin even to discuss the alternative explanation that the vector u reversed really belongs to the solar system, and indicates its velocity, you have practically surrendered to absolute motion. If there were only one star in existence showing relative motion towards the solar system, and someone were to begin to ask: Is it the star that moves or ourselves? we should at once reject the question as absurd, and say that the two alternatives mean the same thing. But I doubt whether there will be many who would be satisfied to contemplate a stellar universe, each member of which has the same relative velocity with respect to the sun, without feeling that here is a problem which requires investigation.

The ideal case considered might be realised if we could imagine observations to be taken from a molecule in a mass of gas enclosed in a box, the observations being taken in the interval between two collisions. If other molecules could be brought within the range of observation, we should, indeed, find that for the mean point in each group containing a large number of molecules the observed relative motion has the same value, that value being the reversed velocity of the molecule itself. If there were any ordinary common-sense philosopher placed on the molecule, he would argue that his observations have really determined the absolute velocity of his place of abode in space, but his wiser colleagues will tell him that he is wrong, and that he has only determined his velocity relative to the rest of the system. The common-sense philosopher would then justly claim that he has done more than that, and point out that the whole system of molecules can be divided into a great many groups, and that the relative velocity in all the groups is identical in magnitude and direction. With our greater knowledge from outside, we know that the velocity which has been determined really belongs to the molecule, and we should probably add (though this is not a matter which the supposed observations could have proved) that it is the velocity relative to the vessel which contains the gas. But even then not all the facts which the observer on the molecule has discovered are accounted for. If the gas could be set into violent motion, without altering the velocity of the particular molecule from which the observations are made, the observer would still obtain the same value for his relative motion, but the different groups into which he has divided his space would no longer give identical values. The complete conclusion to be drawn from the original observations is, that a velocity has been determined relative to an outside body which is mechanically connected with the system, and that each group of molecules is separately at rest relatively to this outside body.

Consider now the application of this example to our problem. If we could argue by analogy we should conclude tnat the observed velocity of the solar system really belongs to it, and not to the stars, but that it must be taken as relative to something which is outside the stellar universe, though connected with it. If that something is material, we should be forced to the conclusion that we have determined a velocity relative to a material body which has not come into our range of observations at all. The conclusion seems too absurd to be entertained. The only alternative is to replace what in the case of the gas was the containing vessel by something immaterial, which therefore we cannot imagine to be in motion. Not being capable of motion it must be at absolute rest, and we may identify it with that abstraction to which we give the name of space. All motion relative to space is absolute motion.

It may be said that if this argument is to be applied to the actual universe, I have left out of account a large number of bodies of which we have no cognisance because they are not luminous. It is possible that these obscure bodies would, if we could observe them, show a systematic motion which would quite upset the previous conclusions. To this I reply that I have treated an ideal case which may or may not coincide with the actual one. I have expressed no opinion as to whether, on the strength of present observations, we are justified in assigning absolute velocity to the solar system; but it may be pointed out that almost every theory which we now believe to be true may some day be upset by facts at present undiscovered, which cannot be reconciled with it. Should we come to the conclusion that our solar system is in motion we may have to modify that belief in the future, but this possibility does not prevent our being justified in adopting views which are in accordance with the facts at present known.

There is an easy way out of the difficulty. When we are driven to our wits' end we have recourse to the æther. Why, then, assume absolute motion when it is so simple to say that you have determined the motion relative to the æther? We should by this device be able to calm our consciences as regards relative motion, no doubt, but at the expense of logical consistency. A moment's reflection will show that the æther has nothing to do with the question. If the æther were non-existent and the corpuscular theory of light were true, the displacement at right angles to the line of sight would still be observed, and as regards the radial velocities the contemplation of the corresponding acoustical problem is decisive. The note of the whistle of an approaching engine is quite independent of the direction of the wind. Similarly, the observed Doppler effect when the spectra of stars are examined is independent of any uniform drift there might be in the æther. No doubt we may, and probably must, consider the æther as immovable in space, and in that case absolute motion in space becomes identical with motion relative to the æther; but the direct conclusion derived from the observation of stars applies to space, and not to the æther.

All our observations of position can only be relative to some standard point. Motion involves change of position, hence motion can be relative only. This is the main argument on which the impossibility of absolute motion is founded. But it proves only that *observed* velocity must be relative, and not that there is nothing real corresponding to absolute velocity. The argument also assumes the very doubtful proposition that velocity must be derived from change of position. It may be the other way round, velocity may be the more fundamental thing, and change of position may have to be derived from it. It seems to be equally logical to take $s=\int v dt$ or $v=ds/dt$ as the equation representing the relationship between the position and velocity. Indeed, if we read Poincaré's description of how we form our ideas of space, we must be struck by the importance attached to the muscular sensation which accompanies a change of position of our bodies. To quote only one of his sentences:—"None of our sensations could have led us by themselves to the conception of space. We are led to this conception only by studying the laws according to which our sensations succeed each other." Bearing this in mind, it seems rational to start from the idea of velocity as rate of change of position, and deduce the idea of position from it. Rest would become an abstraction, and would have to be defined as an infinitely small motion.

There is, however, another way of looking at it. It is known that it is much more difficult to convince ourselves of the objective nature of time than of space or velocity. In fact, I believe most metaphysicians now would deny the objective nature of time altogether. It might therefore be useful to accept both position and velocity as fundamental conceptions, and deduce time intervals from them. In any case, sufficient has been said to show that the practical definition of relative motion cannot in itself be taken to prove that absolute motion is an impossible conception.

If I suggested at the outset of this discussion that most of us have all along been secret believers in absolute motion, I was led to that belief partly by the manner in which the problem of solar motion in space has always been treated. Those who have drawn the logical conclusion from the observations have not infrequently guarded themselves by some statement that after all it is only relative motion they are trying to prove. But their discussion nearly always tacitly assumes, not relative, but absolute motion, and I think most astronomers and physicists, if taken unawares, would admit the absolute motion of the solar system in space as a proved fact, though on second thoughts they might try to explain it away by motion relative to the æther or by some other expedient invented *ad hoc* to safeguard their true faith in relative motion.

But a tacit assumption of absolute motion is also included in the definition of force which at present is much in favour. "Force is rate of change of momentum." If velocity is relative, momentum is relative also. The above definition is therefore incomplete, and may be misleading unless it is definitely stated what the standard system is relative to which momentum has to be measured. Ever quite apart from the earth's rotation the force of gravity impelling a body to the ground is not correctly measured by the rate of change of momentum relative to the earth. Though the neglect of the mass of the body itself, compared with that of the earth, may not lead to serious contradictions in this particular instance, the definition is wrong in principle unless absolute momentum be meant, or unless it be specified that the momentum has to be taken relative to some body unacted on by any forces.

For our definition of force we shall have to return to Newton, and I may have to trouble you with a further communication to justify my belief that though modern criticism has been able to point out the weak spots of the Newtonian system, it has failed to substitute any more secure or more logical basis for our foundation of mechanics. Arthur Schuster.

The Diffusion of Solids.

In view of the interest attaching to the vaporisation and diffusion of solids, the following observations may be worthy of record.

On the inside of the case of a silver watch between forty and fifty years old, and opposite the steel pin of the key-hole, a diffused, dark patch, larger than the key-hole itself, was noticed. When a drop of strong hydrochloric acid was placed on the spot, bubbles of gas were evolved and the colour gradually became lighter, though after ten minutes, when action had apparently ceased, the patch was still plainly marked. On adding a drop of potassium ferrocyanide solution the blue precipitate due to iron was formed. Similar results have been obtained with other old watches. Since it was shown that the iron was not in contact with the silver, the facts indicate that the iron, or possibly some compounds of iron contained in it, vaporises, dissolves in the silver, and penetrates for some distance into the latter by diffusion.

Prof. F. D. Brown has observed an effect of similar nature. On a porcelain writing tablet were notes written in blacklead perhaps forty years ago. While recent writing is easily removed, these marks can no longer be defaced in any way by washing or mere surface scratching, showing that the carbon has penetrated into the porcelain no inconsiderable distance. That this should have taken place in the case of two such refractory solids as carbon and porcelain is all the more remarkable.

John H. Howell.

Grammar School, Auckland, N.Z., February 4.

Earth Tremors in India.

In connection with the short description of the Kangra earthquake, and the reference to the still greater earthquake of 1897, contained in Nature of March 1, p. 418, it may be of interest to note that, at a distance of about twelve miles from the point that was supposed to be the centre of greatest disturbance in the latter earthquake, tremors were still appreciable, at frequent intervals, in the early part of 1904.

While we were sitting on the verandah of the Government bungalow at Rongmudu, in the Garo Hills, near the point at which the river Somersary changes the direction of its flow from east to south, in the early afternoon of February 10, 1904, my travelling companion, Mr. A. B. Nowell, of Dwarra, Sylhêt, directed my attention to a booming sound like the beating of distant gongs, and at the same time pointed to a glass of water standing on the table in front of us, in which the water was distinctly agitated. The tremor lasted for only ten or fifteen seconds.

Mr. Nowell, who had spent some months of each year in that neighbourhood for several years in succession, informed me that tremors occurred at frequent intervals every day when he first came there, but were getting fainter and less frequent as time went on. Later in the same day he directed my attention to another tremor, but as we were then walking in the jungle I failed to appreciate it. On the following day we travelled many miles southwards, or away from the centre of disturbance, so that I had no further opportunity of observing these phenomena.

W. Galloway.

Peculiar Ice Formation.

I should like to direct attention to a peculiar ice formation which I have noticed during the last week on the moorland area at this place, and I should be glad to know if this phenomenon has been observed elsewhere.

The moorland here is of considerable extent, and at a height of 1000 feet above the sea level. The rocks on the upper surface are of a brittle shale with outcropping sandstone, and on the lower slopes beds of clay and gravel. On March 2 I noticed the surface of the ground for distances of a hundred yards or more raised to a height of from 1 to 2 inches, and supported by ice pillars, which had evidently grown by addition of water from below the surface. The sensation of walking on these patches was somewhat analogous to that observed when walking upon a good pile carpet. The late snows had all melted, but the surface contained much moisture, and there had been a certain amount of frost the previous night. The time was 9.30 a.m., and as I stood there these ice pillars crackled and fell in such order as to give the surface a honeycombed appearance.

I found on examination all the talus slopes in the gullies of the moorland to be covered with the same ice structure. These ice pillars were not very evident until some of the earth had been cleared away, as a thin layer of earth was held up in a very uniform manner on the top of them. I found them perpendicular to the surface, both on the pathway and upon the inclined surfaces in the gullies. Several hours afterwards, when the heat of the sun's rays had melted the ice pillars, the whole surface presented a honeycombed appearance. I only noticed this to have taken place in those areas void of any vegetation whatever, and where the heat would be quickly radiated into the atmosphere. The whole of these areas is now broken up into a very fine titrated soil; if this tendency to superficial vertical ice thrust is at all general, it appears to me to be a great factor in the disintegration of surface soils.

James Foulds.

Darwen, Lancashire, March 5.

Cooperation between Scientific Libraries.

In connection with the discussion raised by the note on Dr. Muir's paper (p. 372) and Dr. Bather's letter, it may be of interest to note that the Royal Irish Academy, some five years ago, prepared a classified card-catalogue of the scientific serials accessible in various libraries in Dublin, and it is proposed to keep this up to date through the cooperation of the various librarians. This catalogue is always ready for reference by any member or visitor in the academy's reading-room; and it has been of late years the custom for the library committees of various Dublin institutions to inquire, when a new periodical is proposed, as to its possible previous inclusion in one of the other libraries. Grenville A. J. Cole.

March 10.

Sounding Stones.

With regard to Mr. Tingle's letter in Nature of January 4 (p. 222) on sounding stones, it may interest you to know that I have just seen at Pagan, the former capital of Burma, now in ruins, a large log of fossil (or rather silicified) wood, used as a gong. It emits a clear ringing note when struck, and is used, like all pagoda bells or gongs, to direct the attention of the guardian spirits to the offering about to be presented by the pious Buddhist.

O. F. Wheeler Cuffe.

Meiktila Upper Burma, February 11.

An Inquiry for Books.

Can any reader of Nature direct me to English books on the history of Arabic literature, history of Arabic education, and general sanitation? G. Hammām.

Oriental College, Zahleh, Beirût, Syria, February 24.